

work and easy glory'. In this particular and in other cases, I really did 'bother to check whether older names, presently regarded as synonyms, are available', studying original descriptions and type specimens. This fact does not fit well into Kadolsky's argument and makes the discussion rather bizarre. Of course, old names should always be used instead of creating new ones. In accordance with the Code, however, unused old names should not replace well-known younger ones.

2. I feel that there is some inconsistency in Kadolsky's attitude to facts and objectivity. In his view my 'subjective' comments 'contribute nothing to the solution of the problem, and should not have any bearing'. Why should that be? Subjectivity cannot be avoided in cases like this. The application is, indeed, subjective and Kadolsky's argument, following an initial incorrect conclusion (see para. 3 below), is not itself an example of objectivity (see para. 4 below).

3. It is incorrect to conclude that 'Gittenberger found 25 citations' for the usage of the name *geyeri* from the fact that I listed such a number, and it is odd that my previous clarification concerning this point (BZN 51: 338) has been neglected as if it were untrue. Is there a formal rule, or even a good reason, according to which we should spend time putting together as long as possible a list of references? Should that list be printed? I can imagine more useful activities and ways to spend money.

4. According to Kadolsky, the species *Trochoidea geyeri* is 'still one of the less frequent of the European land snail fauna'. The reasons for this statement are not given, which raises the question on what authority the notion is based. Opposing his view, I reiterate the fact that the specific name is indeed well known. The criteria for being considered well known are given in the Code. I selected references from various languages and disciplines to demonstrate the usage of the name (see para. 3 above). Additionally I referred for authority to *Zoological Record*, Mollusca (1967 and following years). Kadolsky's statement that 'it is easy to obtain such a number of citations [25] even for less important species' is mischievous because it might be incorrectly assumed that only 25 references were found. Apart from this, the implication that there are other, independent criteria to measure the importance of species requires an explanation. Kadolsky's 'belief' concerning the 'audience' is subjectivity par excellence, as are his views concerning the length of names and 'precedence as name based on a little known locality'.

5. Because of conceptual and methodological innovations, systematics has gained new respect among biologists in the scientific world today. We systematists could easily lose that respect by falling prey to prioritisers' dogmatism in nomenclature.

Comment on the proposed conservation of the specific name of *Aplysia juliana* Quoy & Gaimard, 1832 (Mollusca, Gastropoda)

(Case 2949; see BZN 52: 21-23)

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I am writing to support the application to conserve the name *Aplysia juliana* Quoy & Gaimard, 1832 for the sea hare which is found worldwide in warm waters.

The name *Aplysia sorex* Rang, 1828 should be suppressed as it refers to a species dubia. Engel & Eales (1957) and Eales (1960) reported that the specimen identified as *A. sorex* in the Muséum National d'Histoire Naturelle, Paris does not agree with the description of Rang's (1828) *sorex* and is probably not the type. Furthermore, specimens identified by various authors as *sorex* are usually juveniles of *juliana*.

A synonymy for *A. juliana* was given by Eales (1960, p. 363). I listed records of the taxon from the Indian Ocean (Bebbington, 1974) and from the Pacific Ocean (Bebbington, 1977).

Comment on the proposed conservation of the specific names of *Dodecaceria concharum* Örsted, 1843 and *Heterocirrus fimbriatus* Verrill, 1879 (currently *D. fimbriata*) (Annelida, Polychaeta) by the designation of a neotype for *D. concharum*

(Case 2899; see BZN 52: 27-33)

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In their application Gibson & Heppell suggest establishing a neotype for *Dodecaceria concharum*, the type species by monotypy of *Dodecaceria* Örsted, 1843, using a specimen from Cullercoats, Northumberland, England. Two species are present in the Cullercoats area (Garwood, 1982), one of which also occurs in the type locality of east Denmark. Gibson & Heppell's proposal, if accepted, would reserve the name *D. concharum* for the English species which does not occur at the type locality. Although it may be justified to designate a neotype for Örsted's species, we disagree with the choice of locality, and instead argue that it should be selected from topotypic material. Note that, contrary to Gibson & Heppell, only one of Örsted's localities is situated in the Öresund; the area between Fredrikshavn and Skagen is in the northwestern Kattegat.

The proposed selection of neotype locality represents a deliberate misuse of Örsted's name. Further, we question whether their choice of neotype will serve nomenclatural stability. The value of the proposed conservation of British records is not obvious. The taxonomic difficulties in separating species of *Dodecaceria* make it unlikely that the names *concharum* and *fimbriata* (or *caulleryi*) have been used with any great consistency. The consequences for Danish-Swedish records are clear: the use of *concharum* in publications such as Tauber (1879), Levinsen (1884), Thorson (1946), Eliason (1962) and Jägerskiöld (1971), as well as Örsted (1843), will have to be considered incorrect. This despite the admission by Gibson & Heppell that only one species occurs in the area! The Danish species must rightly be referred to as *D. concharum*, not *D. fimbriata*.